

SATURDAY JULY 10, 1880.

UNIFORM TIME.

BY PROFESSOR ORMOND STONE.

It would be a great convenience to the traveling public if all the railroads of the country employed a uniform standard of time. The inconvenience of the present system is so manifest, that it is strange that a united effort has not been made before this to correct it. In so small a country as England the question is a very simple one, but in a country like ours extending over nearly sixty degrees of longitude, or four hours of time, the problem assumes a different aspect owing to the tendency everywhere to employ local time.

In selecting a standard meridian the mind naturally fixes upon Washington as the capitol of the nation and the seat of the largest and best equipped observatory. This meridian passes through the States of New York, Pennsylvania, Maryland, Virginia and North Carolina, and it is hard to imagine what good reason there is that the railroads passing through those States should not run by Washington time. The meridian of Washington is nearly midway between the termini of the three great trunk lines of New York and Pennsylvania. Nevertheless the actual standard employed by each of those roads is clear at one end of the route. Even the Baltimore and Ohio runs by Baltimore instead of Washington time. The meridian thirty minutes west of Washington passes through Michigan, Ohio, Kentucky, Tennessee, Georgia and Florida, and might be adopted as the standard for those States. In four of them it passes very near the capitol or the chief city of the State. In the same manner the roads of Minnesota, Iowa, Missouri, Arkansas and Louisiana might refer their time to a meridian an hour West of Washington; and so on, for the States and Territories lying beyond.

Such a system offers many advantages. The Western Union Telegraph Company transmits Washington time signals each day at noon to nearly all the railroad towns in a large portion of the Union. These signals might be utilized independently of the local time, and the great confusion which now exists would be done away with without greatly violating the preconceived notions of the general public. True noon would still occur within a few minutes of twelve o'clock by the standard time. In addition to the Washington Observatory, two others—the Cincinnati and the Morrison Observatories—have already adopted the standard of time which would accord with this plan, and it is to be hoped that others will follow their example.

WATER SUPPLY OF CITIES.

DR. H. C. H. HEROLD.

The condition of the water supply of cities is now receiving increased attention from sanitary authorities and the medical profession in all countries, especially in England, where the subject has been fully investigated under government authority.

In the series of reports made to the Privy Council of England, Mr. Simon says, "That while it is impossible to make even a rough estimate of the number of persons annually sacrificed by impure water, taking the cases of enteric fever alone, no less than 6,879 deaths occurred recently in one year in England and Wales." In the admirable fifth report, Mr. Simon presents an abstract of no less than one hundred and sixty-four epidemics of typhoid fever, investigated by the department during four years, in all which cases excremental pollution of air or water—generally both—were found to be the cause of the sickness.

It is not my present intention to discuss this question as a whole, but to offer a few facts which will be important to those who rely on wells for their supply of water.

A few weeks since, a large firm whose place of business occupies an entire block in what is called the down-town portion of the city of New York, finding that the use of the water drawn from their well entailed painful results to those who drank it, directed me to make a chemical analysis of a sample of the water taken from the well in question.

The water was clear and sparkling, but had a perceptible bitter taste. The result of the analysis proved to be as follows:

Grains in U. S. Gallon.

Total Residue after evaporation	26.195
Organic or Volatile matter	5.372
Soda (Na O)	3.596
Chlorine (Cl)	5.312
Magnesia (Mg O)	2.635
Sulphuric Acid (SO ₃)	4.247
Lime (Ca O)	4.044
Carbonic Acid (CO ₂)	3.664
Silicic Acid	1.447

The above ingredients are present in the following combinations:

Chloride of Sodium (Na Cl)	6.846
Chloride of Magnesium (Mg Cl ₂)	1.513
Sulphate of Lime, (Ca O, SO ₃)	7.221
Bi-Carbonate of Lime, (Ca O, 2CO ₂)	2.754
Carbonate of Magnesia, 5(Mg O) 4(CO ₂) 6(H ₂ O)	4.354

I need not add, that when this report was made, the use of the well was at once discontinued, for it was palpably unfit for drinking purposes, the large percentage of Chloride of Sodium (nearly 7 grains to the gallon) was decisive of sewage or drainage contamination being present, one grain to the gallon of that substance being considered by the best authorities as the outside limit, for water fit for domestic use.—The large amount of other inorganic impurities will also be noticed by professional readers, for such a large amount of solid residue, would make it a very bad water even for boilers, on account of the Lime, Magnesia and Silica being deposited as a very hard incrustation. Lastly, the analysis shows that the organic matter is largely in excess of what it should be—5.372 grains to the U. S. gallon.

For the purpose of comparison, I offer the result of a chemical analysis of the much abused Croton Water of New York City, made by Professor C. F. Chandler about three years ago, which is as follows:—

Grains in U. S. Gallon.

Soda	0.326
Potassa	0.097
Lime	0.988
Magnesia	0.524
Chlorine	0.243
Sulphuric Acid	0.322
Silica	0.621
Carbonic Acid	2.604
Organic and Volatile Matter	0.670

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VISUAL TELEGRAPHY.

For many years past scientific men have been familiar with the fact, that the reflected images of objects could be reproduced at a distance by the aid of electricity, but recently the matter has been again taken up, and is now being prominently brought before the public under headings of "seeing by telegraph."—

The general principle involved may be gathered from the description of the "*Diaphote*," an instrument introduced by Dr. H. E. Licks of Bethlehem, Penn., "for seeing by telegraph." He calls it by this name "from two Greek words, *dia*, through, and *phos*, light." He lately read a paper in Reading, and exhibited his instrument. This consists of a receiving mirror, the wires, a battery, and a reproducing speculum. The receiving mirror is an amalgam of selenium and iodide of silver; the reproducing speculum is a compound of selenium and chromium. The wires are numerous, as it is necessary for distinctness that a wire should be required to affect but a very small space. The instrument exhibited had a mirror six inches by four, composed of seventy-two small plates to each of which a wire was attached, the whole being wrapped by a fine insulated covering. These wires run to a common galvanic battery and thus connect with the reproducing plate. When the circuit is closed, the rays of light are conducted through an ordinary camera, and the accompanying heat produces chemical changes in the the amalgam of the mirror, which, modifying the electric current, cause similar changes in the reproducing speculum. In the experiments at the close of the explanatory lecture, an instrument was taken to a lower room of the building and operated from there to the stage in the presence of the audience. Before the mirror in the lower room the committee held in succession an apple, a pen knife, and a trade dollar, which were distinct on the platform above. The date on the trade dollar, thrown on an enlarged screen, was plainly visible, as well as the goddess of liberty. A watch was next used, and the audience could see the movement of the hands. An ink bottle, a flower, parts of a theatre hand-bill, were also shown, and when the head of a live kitten was exhibited, there was great applause, and the inventor warmly congratulated on his success. The opinions entertained of its practical value are very high—it being possible for a signal officer on a railroad to see hundreds of miles of track at the same instant."

We are informed by a gentlemen residing near New York, that during a visit to France a few years ago, his attention was called to the successful attempt of the police authorities aided by a Scientist, to reproduce at a distant city by telegraph the features of a criminal who was fleeing from justice. In this case the means employed were perfectly successful, and the results obtained identical to those claimed in the "*Diaphote*."

There are other methods by which "*seeing by tele-*

graph" can be accomplished. Professor Graham Bell has deposited with the Smithsonian Institute a sealed description of an instrument he has invented, which has caused Messrs. Ayrton & Perry of England, who have been working on the same problem, to offer the following statement, which indicates the means they employ:

"While we are still quite in ignorance of the nature of this invention, it may be well to intimate that complete means for seeing by telegraphy have been known for some time by scientific men. The following plan has often been discussed by us with our friends, and no doubt has suggested itself to others acquainted with the physical discoveries of the last four years. It has not been carried out because of its elaborate nature and on account of its expensive character. Nor should we recommend its being carried out.

Our transmitter at A—that is, the apparatus for receiving the light impressions and transmitting them electrically—consisted of a large surface made up of very small separate squares of selenium. One end of each piece was connected by an insulated wire to the distant place, and the other end of each piece with the ground, in accordance with the plan commonly employed with telegraph instruments. The object whose image was to be sent by telegraph was illuminated very strongly, and by means of a lens a very large image was thrown on the surface of the above transmitter. Now, it is well known if each little piece of selenium forms part of a circuit in which there is a constant electromotive force, say of a voltaic battery, the current passing through each piece will depend on its illumination. Hence, the strength of the electric current in each telegraph line will depend on the illumination of its extremity. Our receiver at the other end, B, was, in our original plan, a collection of magnetic needles, the movements of each of which (as in the ordinary needle telegraph) were controlled by the electric current passing through the particular telegraph wire with which it was in connection. Each magnet by its movement closed or opened an aperture through which light passed to illuminate the back of a sheet of frosted glass. There were, of course, as many of the illuminated squares at B as of selenium squares at A, and it is quite evident that since the illumination of each receiving square depends on the strength of the current in its circuit, and this current again depends on the illumination of the selenium at the other end of the wire, the image of a distant object might in this way be transmitted as a mosaic by electricity.

A more promising arrangement, suggested by Professor Kerr's experiments, consisted in having each square at B made of silvered soft iron, and forming the end of the core of a little electro-magnet, round which passed the current, coming from the corresponding selenium square at the other end. We proposed that the surface formed by these squares at B should be illuminated by a great beam of light, polarized by reflection from glass, and received again by an analyzer. It is then evident that since the intensity of the analyzed light depends on the rotation of the plane of polarization, by each little square of iron, and this depends on the strength of the current, and that again on the illumination of the selenium, we have another

method of receiving at B the illumination of the little squares at A."

Even this plan appears to have been anticipated in one sense two years ago, by Mr. J. E. H. Gordon of London, who says:

"I used an electromagnet consisting of an iron bar 2 feet 4 inches long and $2\frac{1}{4}$ inches in diameter, surrounded by 70 lbs. of wire, and excited by ten Grove cells.

The total *double* rotation produced, not by slightly altering the resistance, but by reversing the current, was never more than 26' (twenty-six minutes of arc).

To see this at all with a very delicate Jellett analyzer, it was necessary for the observer to increase the sensitiveness of his eye by sitting in total darkness for some ten minutes before each observation.

Your readers can judge what chance of obtaining visible changes of illumination there would be with 'little' magnets and mere variations in a current not powerful enough to fuse a selenium resistance."

Lastly we may offer an apparatus arranged by Mr. Middleton of Cambridge, England, who gives the following account of it:—

"A lens is used to throw on a plane or suitably curved receiving plate (inclosed in a camera) the image of any object. The receiving plate of the camera is composed of thermopile elements, ground to a smooth surface, and having their posterior faces put in electrical communication by a system of wires, with a somewhat similarly constructed plate. The heating, &c., effect of the image on the first plate generates currents of electricity, which flow through the wire system, and on reaching the second thermopile plate are reconverted into heat, &c., according to the law discovered by Peltier, the amount of heat, &c., being directly proportional to the amount of electricity.

Moreover, according to the manner in which the elements of the plate are arranged with respect to each other, we can get a 'positive' or 'negative' (to use the ordinary phraseology of photography) picture on the second receiving plate, since the Peltier effect here holds, and the copy of a picture depends solely on establishing a constant ratio in the radiant heat and light which corresponding points of the picture and copy send to the eye.

Furthermore, these images can be either viewed directly or by reflected light (after the fashion of the Japanese mirrors and projection on a screen), or by suitable apparatus they can be retained as a photograph, a thermograph, or chemicograph, the details of which will be found in the paper alluded to, and of which an abstract will, I believe, soon appear in the Proceedings of the Cambridge Philosophical Society. Also, I touched upon the method of attacking the problem of photographing in colors, and in conclusion pointed out a striking analogy between the camera of the instrument and that of the human eye; the thermo-electric elements of the instrument and the rods and the cones of the eye; the conducting system of insulated wires emanating from the plate of the instrument and the optic nerve (or bundle of conducting fibres of the eye)—supposing that as the electric currents in the instruments effected a registration on the

sensitive paper, so in the eye the nerve currents of the optic nerve probably leave some brain trace on the mind."

It will thus be seen that while "seeing by telegraph" is not by any means a new invention, the principle involved is one full of interest, and as yet but partially developed; in this field of research ample scope will be found for those working in this direction and valuable results may be anticipated.

THE COMING OF AGE OF THE ORIGIN OF SPECIES.¹

Many of you will be familiar with the aspect of this small green-covered book. It is a copy of the first edition of the "Origin of Species," and bears the date of its production—the first of October, 1859. Only a few months, therefore, are needed to complete the full tale of twenty-one years since its birthday.

Those whose memories carry them back to this time will remember that the infant was remarkably lively, and that a great number of excellent persons mistook its manifestations of a vigorous individuality for mere naughtiness; in fact there was a very pretty turmoil about its cradle. My recollections of the period are particularly vivid; for having conceived a tender affection for a child of what appeared to me to be such remarkable promise, I acted for some time in the capacity of a sort of under-nurse, and thus came in for my share of the storms which threatened even the very life of the young creature. For some years it was undoubtedly warm work, but considering how exceedingly unpleasant the apparition of the new-comer must have been to those who did not fall in love with him at first sight, I think it is to the credit of our age that the war was not fiercer, and that the more bitter and unscrupulous forms of opposition died away as soon as they did.

I speak of this period as of something past and gone, possessing merely a historical, I had almost said an antiquarian, interest. For, during the second decade of the existence of the "Origin of Species," opposition, though by no means dead, assumed a different aspect. On the part of all those who had any reason to respect themselves, it assumed a thoroughly respectful character. By this time the dullest began to perceive that the child was not likely to perish of any congenital weakness or infantile disorder, but was growing into a stalwart personage, upon whom mere goody scoldings and threatenings with the birch-rod were quite thrown away.

In fact, those who have watched the progress of science within the last ten years will bear me out to the full when I assert that there is no field of biological inquiry in which the influences of the "Origin of Species" is not traceable; the foremost men of science in every country are either avowed champions of its leading doctrines, or at any rate abstain from opposing them; a host of young and ardent investigators seek for and find inspiration and guidance in Mr. Darwin's great work; and the general doctrine of Evolution, to one side of which it gives expression, finds in the phenomena of biology a firm base of operations whence it may conduct its conquest of the whole realm of nature.

History warns us, however, that it is the customary fate of new truths to begin as heresies and to end as superstitions; and, as matters now stand, it is hardly rash to anticipate that, in another twenty years, the new generation, educated under the influences of the present day, will be in danger of accepting the main doctrines of the Origin of Species with as little reflection, and it may be with as little justification, as so many of our contemporaries, twenty years ago, rejected them.

Against any such a consummation let us all devoutly pray; for the scientific spirit is of more value than its products, and irrationally-held truths may be more harmful than reasoned errors. Now the essence of the scientific spirit is criticism. It tells us that to whatever doctrine claiming our assent, we should reply, take it if you can compel it. The struggle for existence holds as much in the

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intellectual as in the physical world. A theory is a species of thinking, and its right to exist is coextensive with its power of resisting extinction by its rivals.

From this point of view it appears to me that it would be but a poor way of celebrating the Coming of Age of the Origin of Species were I merely to dwell upon the facts, undoubted and remarkable as they are, of its far-reaching influence and of the great following of ardent disciples who are occupied in spreading and developing its doctrines. Mere insanities and inanities have before now swollen to portentous size in the course of twenty years. Let us rather ask this prodigious change in opinion to justify itself; let us inquire whether anything has happened since 1859 which will explain, on rational grounds, why so many are worshipping that which they burned, and burning that which they worshipped. It is only in this way that we shall acquire the means of judging whether the movement we have witnessed is a mere eddy of fashion, or truly one with the irreversible current of intellectual progress, and, like it, safe from retrogressive reaction.

Every belief is the product of two factors: the first is the state of the mind to which the evidence in favor of that belief is presented; and the second is the logical cogency of the evidence itself. In both these respects the history of biological science during the last twenty years appears to me to afford an ample explanation of the change which has taken place; and a brief consideration of the salient events of that history will enable us to understand why, if the "Origin of Species" appeared now, it would meet with a very different reception from that which greeted it in 1859.

One-and-twenty years ago, in spite of the work commenced by Hutton, and continued with rare skill and patience by Lyell, the dominant view of the past history of the earth was catastrophic. Great and sudden physical revolutions, wholesale creations and extinctions of living beings, were the ordinary machinery of the geological epoch brought in fashion by the misapplied genius of Cuvier. It was gravely maintained and taught that the end of every geological epoch was signalized by a cataclysm, by which every living being on the globe was swept away, to be replaced by a brand-new creation when the world returned to quiescence. A scheme of nature which appeared to be modelled on the likeness of a succession of rubbers of whist, at the end of each of which the players upset the table and called for a new pack, did not seem to shock anybody.

I may be wrong, but I doubt if at the present time there is a single responsible representative of these opinions left. The progress of scientific geology has elevated the fundamental principle of uniformitarianism, that the explanation of the past is to be sought in the study of the present, into the position of an axiom; and the wild speculations of the catastrophists, to which we all listened with respect a quarter of a century ago, would hardly find a single patient hearer at the present day. No physical geologist now dreams of seeking outside the ranges of known natural causes for the explanation of anything that happened millions of years ago, any more than he would be guilty of the like absurdity in regard to current events.

The effect of this change of opinion upon biological speculation is obvious. For, if there have been no periodical general physical catastrophes, what brought about the assumed general extinctions and re-creations of life which are the corresponding biological catastrophes? And if no such interruptions of the ordinary course of nature have taken place in the organic, any more than in the inorganic world, what alternative is there to the admission of Evolution?

The doctrine of Evolution in Biology is the necessary result of the logical application of the principles of uniformitarianism to the phenomena of life. Darwin is the natural successor of Hutton and Lyell, and the "Origin of Species" the natural sequence of the "Principles of Geology."

The fundamental doctrine of the "Origin of Species," as of all forms of the theory of Evolution applied to biology, is "that the innumerable species, genera, and families of organic beings with which the world is peopled have all descended, each within its own class or group, from common parents, and have all been modified in the course of descent."¹

And, in view of the facts of geology, it follows that all living animals and plants "are the lineal descendants of those which lived long before the Silurian epoch."¹

It is an obvious consequence of this theory of Descent with Modification, as it is sometimes called, that all plants and animals, however different they may now be, must, at one time or other, have been connected by direct or indirect intermediate gradations, and that the appearance of isolation presented by various groups of organic beings must be unreal.

No part of Mr. Darwin's work ran more directly counter to the prepossessions of naturalists twenty years ago than this. And such prepossessions were very excusable, for there was undoubtedly a great deal to be said, at that time, in favor of the fixity of species and of the existence of great breaks, which there was no obvious or probable means of filling up, between various groups of organic beings.

For various reasons, scientific and unscientific, much had been made of the hiatus between man and the rest of the higher mammalia, and it is no wonder that issue was first joined on this part of the controversy. I have no wish to revive past and happily forgotten controversies, but I must state the simple fact that the distinctions in cerebral and other characters, which were so hotly affirmed to separate man from all other animals in 1860, have all been demonstrated to be non-existent, and that the contrary doctrine is now universally accepted and taught.

But there were other cases in which the wide structural gaps asserted to exist between one group of animals and another were by no means fictitious; and, when such structural breaks were real, Mr. Darwin could account for them only by supposing that the intermediate forms which once existed had become extinct. In a remarkable passage he says:—

"We may thus account even for the distinctness of whole classes from each other—for instance of birds from all other vertebrate animals—by the belief that many animal forms of life have been utterly lost, through which the early progenitors of birds were formerly connected with the early progenitors of the other vertebrate classes."²

Adverse criticism made merry over such suggestions as these. Of course it was easy to get out of the difficulty by supposing extinction; but where was the slightest evidence that such intermediate forms between birds and reptiles as the hypothesis required ever existed? And then probably followed a tirade upon this terrible forsaking of the paths of "Baconian induction."

But the progress of knowledge has justified Mr. Darwin to an extent which could hardly have been anticipated. In 1862, the specimen of *Archæopteryx*, which until the last two or three years has remained unique, was discovered; and it is an animal which, in its feathers and the greater part of its organization, is a veritable bird, while, in other parts, it is as distinctly reptilian.

In 1868, I had the honour of bringing under your notice, in this theatre, the results of investigations made, up to that time, into the anatomical characters of certain ancient reptiles, which showed the nature of the modifications in virtue of which the type of the quadrupedal reptile passed into that of the bipedal bird; and abundant confirmatory evidence of the justice of the conclusions which I then laid before you has since come to light.

In 1875, the discovery of the toothed birds of the cretaceous formation in North America, by Prof. Marsh, completed the series of transitional forms between birds and reptiles, and removed Mr. Darwin's proposition that "many animal forms of life have been utterly lost, through which the early progenitors of birds were formerly connected with the early progenitors of the other vertebrate classes," from the region of hypothesis to that of demonstrable fact.

In 1859, there appeared to be a very sharp and clear hiatus between vertebrate and invertebrate animals, not only in their structure, but, what was more important, in their development. I do not think that we even yet know the precise links of connection between the two; but the investigations of Kowalewsky and others upon the develop-

¹ "Origin of Species," ed. 1, p. 457.

¹ "Origin of Species," ed. 1, p. 458.

² "Origin of Species," ed. 1, p. 431.

ment of *Amphioxus* and of the *Tunicata* prove beyond a doubt that the differences which were supposed to constitute a barrier between the two are non-existent. There is no longer any difficulty in understanding how the vertebrate type may have arisen from the invertebrate, though the full proof of the manner in which the transition was actually effected may still be lacking.

Again, in 1859, there appeared to be a no less sharp separation between the two great groups of flowering and flowerless plants. It is only subsequently that the series of remarkable investigations inaugurated by Hofmeister has brought to light the extraordinary and altogether unexpected modifications of the reproductive apparatus in the *Lycopodiaceæ*, the *Rhizocarpeæ*, and the *Gymnospermeæ*, by which the ferns and the mosses are gradually connected with the Phanerogamic division of the vegetable world.

So, again, it is only since 1859 that we have acquired that wealth of knowledge of the lowest forms of life which demonstrates the futility of any attempt to separate the lowest plants from the lowest animals, and shows that the two kingdoms of living nature have a common borderland which belongs to both or to neither.

Thus it will be observed that the whole tendency of biological investigation since 1859 has been in the direction of removing the difficulties which the apparent breaks in the series created at that time; and the recognition of gradation is the first step towards the acceptance of evolution.

As another great factor in bringing about the change of opinion which has taken place among naturalists, I count the astonishing progress which has been made in the study of embryology. Twenty years ago, not only were we devoid of any accurate knowledge of the mode of development of many groups of animals and plants, but the methods of investigation were rude and imperfect. At the present time there is no important group of organic beings the development of which has not been carefully studied, and the modern methods of hardening and section-making enable the embryologist to determine the nature of the process in each case, with a degree of minuteness and accuracy which is truly astonishing to those whose memories carry them back to the beginnings of modern histology. And the results of these embryological investigations are in complete harmony with the requirements of the doctrine of evolution. The first beginnings of all the higher forms of animal life are similar, and however diverse their adult conditions, they start from a common foundation. Moreover the process of development of the animal or the plant from its primary egg or germ is a true process of evolution—a process from almost formless to more or less highly organized matter, in virtue of the properties inherent in that matter.

To those who are familiar with the process of development all *à priori* objections to the doctrine of biological evolution appear childish. Any one who has watched the gradual formation of a complicated animal from the protoplasmic mass which constitutes the essential element of a frog's or a hen's egg has had under his eyes sufficient evidence that a similar evolution of the animal world from the like foundation is, at any rate, possible.

Yet another product of investigation has largely contributed to the removal of the objections to the doctrine of Evolution current in 1859. It is the proof afforded by successive discoveries that Mr. Darwin did not overestimate the imperfection of the geological record. No more striking illustration of this is needed than a comparison of our knowledge of the mammalian fauna of the Tertiary epoch in 1859 with its present condition. M. Gaudry's researches on the fossils of Pikermi were published in 1868, those of Messrs. Leidy, Marsh, and Cope on the fossils of the Western Territories of America, have appeared almost wholly since 1870; those of M. Filhol, on the phosphorites of Quercy, in 1878. The general effect of these investigations has been to introduce us to a multitude of extinct animals, the existence of which was previously hardly suspected; just as if zoologists were to become acquainted with a country, hitherto unknown, as rich in novel forms of life, as Brazil or South Africa once were to Europeans. Indeed the fossil fauna of the Western Territories of America bids fair to exceed in interest and importance

all other known Tertiary deposits put together; and yet, with the exception of the case of the American Tertiaries, these investigations have extended over very limited areas, and at Pikermi were confined to an extremely small space.

Such appear to me to be the chief events in the history of the progress of knowledge, during the last twenty years, which account for the changed feeling with which the doctrine of Evolution is at present regarded by those who have followed the advance of biological science in respect of those problems which bear indirectly upon that doctrine.

But all this remains mere secondary evidence. It may remove dissent, but it does not compel assent. Primary and direct evidence in favor of Evolution can be furnished only by palæontology. The geological record, so soon as it approaches completeness, must, when properly questioned, yield either an affirmative or negative answer; if evolution has taken place, there will its mark be left; if it has not taken place, there will lie its refutation.

What was the state of matters in 1859? Let us hear Mr. Darwin, who may be trusted always to state the case against himself as strongly as possible.

"On this doctrine of the extermination of an infinitude of connecting links between the living and extinct inhabitants of the world, and at each successive period between the extinct and still older species, why is not every geological formation charged with such links? Why does not every collection of fossil remains afford plain evidence of the gradation and mutation of the forms of life? We meet with no such evidence, and this is the most obvious and plausible of the many objections which may be urged against my theory."¹

Nothing could have been more useful to the opposition than this characteristically candid avowal, twisted as it immediately was into an admission that the writer's views were contradicted by the facts of palæontology. But, in fact, Mr. Darwin made no such admission. What he says in effect is, not that palæontological evidence is against him, but that it is not distinctly in his favor; and without attempting to attenuate the fact, he accounts for it by the scantiness and the imperfection of that evidence.

What is the state of the case now, when, as we have seen, the amount of our knowledge respecting the mammalia of the Tertiary epoch is increased fifty-fold, and in some directions even approaches completeness?

Simply this, that if the doctrine of Evolution has not existed palæontologists must have invented it, so irresistibly is it forced upon the mind by the study of the remains of the Tertiary mammalia which have been brought to light since 1859.

Among the fossils of Pikermi, Gaudry found the successive stages by which the ancient civets passed into the more modern hyænas; through the Tertiary deposits of Western America, Marsh tracked the successive forms by which the ancient stock of the horse has passed into its present form; and innumerable less complete indications of the mode of evolution of other groups of the higher mammalia have been obtained.

In the remarkable memoir on the Phosphorites of Quercy, to which I have referred, M. Filhol describes no fewer than seventeen varieties of the genus *Cynodontis*, which fill up all the interval between the viverrine animals and the bear-like dog *Amphicyon*; nor do I know any solid ground of objection to the supposition that in this *Cynodontis-Amphicyon* group we have the stock whence all the *Viverridæ*, *Felidæ*, *Hyænidæ*, *Canidæ*, and perhaps the *Procyonidæ* and *Ursidæ*, of the present fauna have been evolved. On the contrary, there is a great deal to be said in its favor.

In the course of summing up his results, M. Filhol observes² :—

"During the epoch of the phosphorites, great changes took place in animal forms, and almost the same types as those which now exist became defined from one another.

Under the influence of natural conditions of which we have no exact knowledge, though traces of them are discoverable, species have been modified in a thousand ways; races have arisen which, becoming fixed, have

(Concluded on page 20.)

¹ "Origin of Species, ed. 1, p. 463.

² This passage was omitted in the delivery of the lecture.

thus produced a corresponding number of secondary species."

In 1859, language of which this is an unintentional paraphrase, occurring in the "Origin of Species," was scouted as wild speculation; at present, it is a sober statement of the conclusions to which an acute and critically-minded investigator is led by large and patient study of the facts of palæontology. I venture to repeat what I have said before, that, so far as the animal world is concerned, Evolution is no longer a speculation, but a statement of historical fact. It takes its place alongside of those accepted truths which must be taken into account by philosophers of all schools.

Thus when, on the first day of October next, the "Origin of Species" comes of age, the promise of its youth will be amply fulfilled; and we shall be prepared to congratulate the venerated author of the book, not only that the greatness of his achievement and its enduring influence upon the progress of knowledge have won him a place beside our Harvey; but, still more, that, like Harvey, he has lived long enough to outlast detraction and opposition, and to see the stone that the builders rejected become the headstone of the corner.

T. H. HUXLEY.

WATER ANALYSIS.

The recent publication of Dr. Frankland's convenient little volume on this subject;* the important memoir by Dr. Tidy read and discussed last year before the Chemical Society, and published in its *Journal*;† and the volume published as long ago as 1868, and now appearing in a fifth edition, by Professor Wanklyn and Mr Chapman,‡ in which Dr. Frankland's title was anticipated, contains the literature of a subject which has of late years assumed extreme importance, but concerning which there is a haze of mystery and obscurity that assuredly ought not to exist. It is certainly very much to be regretted that there is no common and recognized method of procedure in this department of chemistry, but it is almost discreditable that not only are the results of analyses given in discordant chemical expressions, but even the figures are not in the same terms, so that a comparison of results is impossible without performing an arithmetical operation. So long as one chemist expresses his results in 100-1000th parts, another in grains per gallon, and a third in milligrammes, or parts in a million; while one estimates ammonia as a total, another separates free from organic ammonia, and the third regards the quantity of what is called albumenoid ammonia as of vital importance; while one adopts the combustion or evaporation method to determine the actual quantity of organic contents of water, and another accepts the permanganate method to discover the quantity of oxygen required to oxidize the organic matter present; it is evident that the comparison of analyses affected by the pupils of the various schools cannot be satisfactory or conclusive, because they cannot be compared. Surely the time has come when methods of analysis giving the quantity of organic carbon and nitrogen and its condition or history in some intelligible form, the quantity of nitrogen as nitrates, the quantity of chlorine, and the hardness, in similar terms, should be so far agreed upon that results can be compared, and those who are not chemists will then be able to form some opinion as to facts. We believe all our most distinguished chemists would agree that this is possible. It only needs that each should give way in some matters that are not essential, but rather belong to the

individual. Dr. Tidy well and properly observes in his paper (*Journal of the Chemical Society*, Jan., 1879): "I am afraid the public have taken note and are taking note of chemists' differences, and distrust our work accordingly. Nor indeed is their distrust to be wondered at, deeply as it is to be lamented."

In Dr. Frankland's book we find stated, in a compact and convenient form, the requirements of a water analysis, and in an appendix examples of typical analyses. He begins by pointing out the fact that complete and ultimate analyses are by no means called for in ordinary cases. He points out the unimportance, in a sanitary sense, of the dissolved gases, which vary but little in waters of very different kinds, and of which the presence of a smaller or larger quantity does not affect the goodness of the water; the difference, in fact, lying chiefly in the quantity of carbonic acid. The separate estimation of the quantity of each of the saline matters and of each organic constituent of the suspended matters, may in like manner, and for the same reason, be omitted. The processes adopted to determine the quantities of inorganic solids, the ammonia, the chlorine, the nature of the hardening ingredients, and the presence of poisonous metals, if any, are those which are really important, and a knowledge of them and of the amount of nitrates, and lastly, but of chief importance, the means of estimating approximately the proportion of the organic elements in a sample of water, are the objects of which attention is really required, and to the elucidation to which the volume is dedicated. Professor Frankland considers that there is no process, short of the actual combustion of the organic matter present in water, which affords thoroughly trustworthy evidence of the organic carbon and nitrogen, and of the fitness or otherwise of the sample for dietetic purposes. The "ignition" and "albumenoid ammonia" he merely mentions, and evidently disregards. The former is described by Dr. Tidy in his memoir, and he considers it is not satisfactory, as failing to show that, in carrying out the process—(1) no organic matter is lost, (2) that all the organic matter is burnt off, and (3) that no organic matter is added. Notwithstanding this, he adopts it in the analysis of sewerage, and thinks that in some respects it may be indicative, and suggestive in other cases. The ammonia process, described by Mr. Wanklyn as "a sort of combustion process, with ammonia for the ultimate product," has for its object the comparative determination of the nitrogenous organic matter by the quantity of ammonia yielded by the destruction of the organic matter, this quantity being called "albumenoid ammonia." (Wanklyn's "Water Analysis," 5th Ed., p. 31.) Dr. Tidy has considered in detail the advantages and disadvantages of this method and has given some remarkable illustrations of its failure in important cases. He points out the very important fact that the quantity of albumenoid ammonia in peaty water is very large, although it has never been proved that such water is in any sense injurious; and, on the other hand, that in waters regarded by Mr. Wanklyn as exceedingly bad, the albumenoid ammonia is almost *nil*. For these reasons apparently, as he quotes Dr. Tidy's paper and gives no other reference, Dr. Frankland rejects them.

In the commencement of this work, following Mr. Wanklyn in this, Dr. Frankland describes the preliminary considerations in water sampling, the quantity required, and the tests that should be applied to determine the presence of mineral poisons, the nature of refuse from manufactures, the action on soft lead, and the cause of turbidity. Having thus opened the subject, he proceeds to show in what way the total solids in solution can be best determined. To determine the organic contents, he prefers the combustion method. He describes the precautions required in evaporation, and believes that "the proportion of solid residue left on evaporation affords an approximate, though somewhat rough indication of the comparative purity of water." This, no doubt, is true in a certain sense, although it must not be concluded that waters showing a large residue are necessarily bad. It is with water as with many other things, we must be content with the best we can obtain under existing circumstances, and absolute purity is practically unobtainable. A tolerably good river water at hand is often better than deep well water or lake water from a distance, though theoretically superior.

* "Water Analysis for Sanitary Purposes, with Hints for the Interpretation of Results." By E. Frankland, Ph.D., F.R.S., &c. London: Van Voorst. 1880.

† "The Processes for Determining the Organic Purity of Potable Waters." By C. Meymott Tidy, M.B. *Journal of the Chemical Society*, Jan., 1879.

‡ "Water Analysis: a Practical Treatise on the Examination of Potable Water." By J. Alfred Wanklyn, M.R.C.S., and Ernest Theophrastus Chapman. London: Thribner & Co. Fifth Edition. 1879.

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JOHN MICHELS, Editor.

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AN exhibition is now in progress at Clarendon Hall, New York City, of a Dr. Tanner, who claims to be existing without any nourishment, but that derived from rinsing his mouth occasionally with cold water. At the date of our seeing Dr. Tanner, eight days of such fasting were supposed to have been carried out. The fact that Dr. Tanner has declined those rigid conditions prescribed by Dr. William A. Hammond, which would have made fraud impossible, and is watched merely by persons of his own selection, deprives the so-called experiment of scientific interest. Under these circumstances a judgment of the case is impossible; but a critical observation of Dr. Tanner and his surroundings was very unfavorable to the genuineness of the proceedings. During the fifteen minutes we remained, two opportunities were presented for Dr. Tanner to take food unobserved by his watchers—once when he ordered the light to be removed, and the room was made sufficiently dark to obscure his movements; and again when Dr. Tanner left the room and reclined on the top of the portico; during this time the watchers were in another room, and had we been confederates of Dr. Tanner, we could have handed him food unobserved.

WE are informed that the Earl of Rosse desires the services of an Assistant Astronomer for his well-known Observatory. The address of Lord Rosse is Birr Castle, Parsonstown, Ireland.

On the shores of the Lake of Constance the rare phenomenon of a perfect solar halo was noticed on May 4, at noon. The large ring, which from time to time assumed splendid rainbow tints, remained visible for more than two hours. At Berlin the phenomenon of mock suns was observed on the 9th inst., at 8 A.M.

The *Proceedings* of the Davenport Academy of Natural Sciences, while it contains a number of papers in natural history, is noteworthy mainly for the large number of papers on subjects connected with American ethnology, and chiefly on various mound explorations. We are pleased to see that this society continues to prosper. It had the originality to elect as its president for 1879 Mrs. Mary L. D. Putnam.

In the Transactions of the Academy of Science of St. Louis are several papers deserving attention. Mr. N. Holmes has a specially interesting paper on the "Geological and Geographical Distribution of the Human Race," and students of the science of language will be interested in M. Coruna y Collado's account of the Zoque language, spoken in the State of Chiapas, Mexico. There are two magnetic papers by Prof. Nipher; a paper on *Pentremites* by Dr. G. Hanbach; on the genus *Pinus* by M. G. Englemann, who has also a short paper on Acorns and their Germination.

Mr. C. S. Sargent, Harvard Professor of Arboriculture, has published, in his capacity of Special Agent of the approaching United States Census, a "Catalogue of the Forest Trees of North America," preliminary to one which will be added to the census report on the forest wealth of the United States. He desires information concerning the geographical range of any species; the most favorable region and elevation and geographical formation for its multiplication and perfection; its exceptionally large dimensions; its common or local name; and its products and uses.

Professor C. G. Rockwood, of Princeton, who has for some time devoted his attention to the study of earthquakes and volcanic phenomena, desires to come into communication with other workers in the field of Seismology, either at home or abroad. He is especially desirous to obtain correspondents upon the Pacific Coast, who would aid in collecting information in regard to earthquake shocks in that region, where they are so much more frequent than on the Atlantic seaboard. Professor C. W. Fuchs, formerly of Heidelberg, but now of Merin, Tyrol, Austria, has for the last fifteen years published annual statistics of earthquakes, and as it is clear from these lists, that but few cases of such phenomena in America are noted, we trust our readers will respond to the present request of Professor Rockwood.

FARADAY'S OBSERVATIONS ON SILVERING GLASS.

1,540 grains of nitrate of silver being treated with 955 grains of strong solution of ammonia, and afterwards with 7,700 grains of water, yields a solution to which, when clear, 170 grains of tartaric acid, dissolved in 680 grains of water, is to be added, and then 152 cubic inches more of water, with good agitation. When the liquid has settled, the clear part is to be poured off; 152 cubic inches of water to be added to the remaining solid matter, that as much may be dissolved as possible; and the clear fluids to be put together and increased by the further addition of 61 cubic inches of water. This is the silvering solution, No. 1. A second fluid, No. 2, is to be prepared in like manner, with this difference, that the tartaric acid is to be doubled in quantity. The apparatus employed for the silvering of glass plate consists of a cast iron table box, containing water within, and a set of gas burners beneath to heat it; the upper surface of the table is planed, and set truly horizontal by a level and covered by a varnished cloth; heat is applied until the temperature is 140 deg. Fah. The glass is well cleaned, first with a cloth; after which a plug of cotton, dipped in the silvering fluid and a little polishing powder, is carefully passed over the surface to be silvered, and when this application is dry it is removed by another plug of cotton, and the plate is perfectly clean. The glass is then laid on the table, a portion of the silvering fluid poured on to the surface, and this spread carefully over every part by a cylinder of india-rubber stretched upon wood which has previously been cleaned and wetted with the solution; in this manner a perfect wetting of the service is obtained, and all air bubbles, &c., are removed. Then more fluid is poured on to the glass, until it is covered with a layer about the one-tenth of an inch in depth, which easily stands upon it, and in that state its temperature is allowed to rise. In about ten minutes or more silver begins to deposit on the glass, and in fifteen or twenty minutes a uniform opaque coat, having a greyish tint on the upper surface, is deposited. After a certain time the glass employed in the illustration was pushed to the edge of the table, was tilted that the fluid might be poured off, then washed with water, and examined. The under surface presented a perfectly brilliant metallic plate of high reflective power, as high as silver can attain to; and the coat of silver, though thin, was so strong as to bear handling, and so firm as to stand polishing on the back to any degree, by rubbing with the hand and polishing powder. The usual course in practice, however, is—when the first stratum of fluid is exhausted—to remove it, and apply a layer of No. 2 solution; and when that has been removed, and the glass washed and dried, to cover the back surface with a protective coat of black varnish. When the form of the glass varies, simple expedients are employed, and by their means either concave, convex, or corrugated surfaces are silvered, and bottles and vases coated internally. It is easy to repair an injury in the silvering of a plate.

The advantages are—the production of a perfect reflecting surface; the ability to repair; the mercantile economy of the process (the silver in a square yard of surface being worth only 1s 8d); the certainty, simplicity, and quickness of the operation; and, above all, the non-employment of mercury.

AN exhibition is now in progress at Clarendon Hall, New York City, of a Dr. Tanner, who claims to be existing without any nourishment, but that derived from rinsing his mouth occasionally with cold water. At the date of our seeing Dr. Tanner, eight days of such fasting were supposed to have been carried out. The fact that Dr. Tanner has declined those rigid conditions prescribed by Dr. William A. Hammond, which would have made fraud impossible, and is watched merely by persons of his own selection, deprives the so-called experiment of scientific interest. Under these circumstances a judgment of the case is impossible; but a critical observation of Dr. Tanner and his surroundings was very unfavorable to the genuineness of the proceedings. During the fifteen minutes we remained, two opportunities were presented for Dr. Tanner to take food unobserved by his watchers—once when he ordered the light to be removed, and the room was made sufficiently dark to obscure his movements; and again when Dr. Tanner left the room and reclined on the top of the portico; during this time the watchers were in another room, and had we been confederates of Dr. Tanner, we could have handed him food unobserved.

WE are informed that the Earl of Rosse desires the services of an Assistant Astronomer for his well-known Observatory. The address of Lord Rosse is Birr Castle, Parsonstown, Ireland.

On the shores of the Lake of Constance the rare phenomenon of a perfect solar halo was noticed on May 4, at noon. The large ring, which from time to time assumed splendid rainbow tints, remained visible for more than two hours. At Berlin the phenomenon of mock suns was observed on the 9th inst., at 8 A.M.

The *Proceedings* of the Davenport Academy of Natural Sciences, while it contains a number of papers in natural history, is noteworthy mainly for the large number of papers on subjects connected with American ethnology, and chiefly on various mound explorations. We are pleased to see that this society continues to prosper. It had the originality to elect as its president for 1879 Mrs. Mary L. D. Putnam.

In the Transactions of the Academy of Science of St. Louis are several papers deserving attention. Mr. N. Holmes has a specially interesting paper on the "Geological and Geographical Distribution of the Human Race," and students of the science of language will be interested in M. Coruna y Collado's account of the Zoque language, spoken in the State of Chiapas, Mexico. There are two magnetic papers by Prof. Nipher; a paper on *Pentremites* by Dr. G. Hanbach; on the genus *Pinus* by M. G. Englemann, who has also a short paper on Acorns and their Germination.

Mr. C. S. Sargent, Harvard Professor of Arboriculture, has published, in his capacity of Special Agent of the approaching United States Census, a "Catalogue of the Forest Trees of North America," preliminary to one which will be added to the census report on the forest wealth of the United States. He desires information concerning the geographical range of any species; the most favorable region and elevation and geographical formation for its multiplication and perfection; its exceptionally large dimensions; its common or local name; and its products and uses.

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thus produced a corresponding number of secondary species."

In 1859, language of which this is an unintentional paraphrase, occurring in the "Origin of Species," was scouted as wild speculation; at present, it is a sober statement of the conclusions to which an acute and critically-minded investigator is led by large and patient study of the facts of palæontology. I venture to repeat what I have said before, that, so far as the animal world is concerned, Evolution is no longer a speculation, but a statement of historical fact. It takes its place alongside of those accepted truths which must be taken into account by philosophers of all schools.

Thus when, on the first day of October next, the "Origin of Species" comes of age, the promise of its youth will be amply fulfilled; and we shall be prepared to congratulate the venerated author of the book, not only that the greatness of his achievement and its enduring influence upon the progress of knowledge have won him a place beside our Harvey; but, still more, that, like Harvey, he has lived long enough to outlast detraction and opposition, and to see the stone that the builders rejected become the headstone of the corner.

T. H. HUXLEY.

WATER ANALYSIS.

The recent publication of Dr. Frankland's convenient little volume on this subject;* the important memoir by Dr. Tidy read and discussed last year before the Chemical Society, and published in its *Journal*;† and the volume published as long ago as 1868, and now appearing in a fifth edition, by Professor Wanklyn and Mr Chapman,‡ in which Dr. Frankland's title was anticipated, contains the literature of a subject which has of late years assumed extreme importance, but concerning which there is a haze of mystery and obscurity that assuredly ought not to exist. It is certainly very much to be regretted that there is no common and recognized method of procedure in this department of chemistry, but it is almost discreditable that not only are the results of analyses given in discordant chemical expressions, but even the figures are not in the same terms, so that a comparison of results is impossible without performing an arithmetical operation. So long as one chemist expresses his results in 100-1000th parts, another in grains per gallon, and a third in milligrammes, or parts in a million; while one estimates ammonia as a total, another separates free from organic ammonia, and the third regards the quantity of what is called albumenoid ammonia as of vital importance; while one adopts the combustion or evaporation method to determine the actual quantity of organic contents of water, and another accepts the permanganate method to discover the quantity of oxygen required to oxidize the organic matter present; it is evident that the comparison of analyses affected by the pupils of the various schools cannot be satisfactory or conclusive, because they cannot be compared. Surely the time has come when methods of analysis giving the quantity of organic carbon and nitrogen and its condition or history in some intelligible form, the quantity of nitrogen as nitrates, the quantity of chlorine, and the hardness, in similar terms, should be so far agreed upon that results can be compared, and those who are not chemists will then be able to form some opinion as to facts. We believe all our most distinguished chemists would agree that this is possible. It only needs that each should give way in some matters that are not essential, but rather belong to the

individual. Dr. Tidy well and properly observes in his paper (*Journal of the Chemical Society*, Jan., 1879): "I am afraid the public have taken note and are taking note of chemists' differences, and distrust our work accordingly. Nor indeed is their distrust to be wondered at, deeply as it is to be lamented."

In Dr. Frankland's book we find stated, in a compact and convenient form, the requirements of a water analysis, and in an appendix examples of typical analyses. He begins by pointing out the fact that complete and ultimate analyses are by no means called for in ordinary cases. He points out the unimportance, in a sanitary sense, of the dissolved gases, which vary but little in waters of very different kinds, and of which the presence of a smaller or larger quantity does not affect the goodness of the water; the difference, in fact, lying chiefly in the quantity of carbonic acid. The separate estimation of the quantity of each of the saline matters and of each organic constituent of the suspended matters, may in like manner, and for the same reason, be omitted. The processes adopted to determine the quantities of inorganic solids, the ammonia, the chlorine, the nature of the hardening ingredients, and the presence of poisonous metals, if any, are those which are really important, and a knowledge of them and of the amount of nitrates, and lastly, but of chief importance, the means of estimating approximately the proportion of the organic elements in a sample of water, are the objects of which attention is really required, and to the elucidation to which the volume is dedicated. Professor Frankland considers that there is no process, short of the actual combustion of the organic matter present in water, which affords thoroughly trustworthy evidence of the organic carbon and nitrogen, and of the fitness or otherwise of the sample for dietetic purposes. The "ignition" and "albumenoid ammonia" he merely mentions, and evidently disregards. The former is described by Dr. Tidy in his memoir, and he considers it is not satisfactory, as failing to show that, in carrying out the process—(1) no organic matter is lost, (2) that all the organic matter is burnt off, and (3) that no organic matter is added. Notwithstanding this, he adopts it in the analysis of sewerage, and thinks that in some respects it may be indicative, and suggestive in other cases. The ammonia process, described by Mr. Wanklyn as "a sort of combustion process, with ammonia for the ultimate product," has for its object the comparative determination of the nitrogenous organic matter by the quantity of ammonia yielded by the destruction of the organic matter, this quantity being called "albumenoid ammonia." (Wanklyn's "Water Analysis," 5th Ed., p. 31.) Dr. Tidy has considered in detail the advantages and disadvantages of this method and has given some remarkable illustrations of its failure in important cases. He points out the very important fact that the quantity of albumenoid ammonia in peaty water is very large, although it has never been proved that such water is in any sense injurious; and, on the other hand, that in waters regarded by Mr. Wanklyn as exceedingly bad, the albumenoid ammonia is almost *nil*. For these reasons apparently, as he quotes Dr. Tidy's paper and gives no other reference, Dr. Frankland rejects them.

In the commencement of this work, following Mr. Wanklyn in this, Dr. Frankland describes the preliminary considerations in water sampling, the quantity required, and the tests that should be applied to determine the presence of mineral poisons, the nature of refuse from manufactures, the action on soft lead, and the cause of turbidity. Having thus opened the subject, he proceeds to show in what way the total solids in solution can be best determined. To determine the organic contents, he prefers the combustion method. He describes the precautions required in evaporation, and believes that "the proportion of solid residue left on evaporation affords an approximate, though somewhat rough indication of the comparative purity of water." This, no doubt, is true in a certain sense, although it must not be concluded that waters showing a large residue are necessarily bad. It is with water as with many other things, we must be content with the best we can obtain under existing circumstances, and absolute purity is practically unobtainable. A tolerably good river water at hand is often better than deep well water or lake water from a distance, though theoretically superior.

* "Water Analysis for Sanitary Purposes, with Hints for the Interpretation of Results." By E. Frankland, Ph.D., F.R.S., &c. London: Van Voorst. 1880.

† "The Processes for Determining the Organic Purity of Potable Waters." By C. Meymott Tidy, M.B. *Journal of the Chemical Society*, Jan., 1879.

‡ "Water Analysis: a Practical Treatise on the Examination of Potable Water." By J. Alfred Wanklyn, M.R.C.S., and Ernest Theophrastus Chapman. London: Thribner & Co. Fifth Edition. 1879.

In speaking of quality, Dr. Frankland adheres throughout to the strict technical use of the words "impurity" and "pollution," applying them to all foreign substances present in water. This is to be regretted, as they are eminently misleading when referring to the quality of water for ordinary purposes, and are certain to foster prejudices both unfair and mischievous. According to his use of these terms, all mineral waters, including those especially recommended for dietetic purposes, might be quoted as exceedingly impure, and loaded with polluting matter.

The determination of the ammonia is the next point considered. It is admitted that the actual quantity of ammonia present is, of itself no guide to the purity of water, as there are many cases of deep-well water in which the quantity is large, though there has been no access of animal matter. As, however, ammonia in water is very commonly caused by animal matter in a state of incipient decomposition, and is found in water polluted by sewerage, in shallow well water, and in some cases in river water, the quantity present in a given quantity of water is regarded as an essential inquiry, and its presence is suggestive of evil.

Chlorine is present in water chiefly as a constituent of common salt, and this is so uniformly found in the liquid excrement of animals, that its presence in water is also to be distrusted. At the same time, a certain quantity is certainly washed out of the air and soil by rain, although the proportion of this is not constant. The quantities allowed by Dr. Frankland as due to these causes are 0.22 per 100,000 for rain water, 1.13 for upland surface water, 2.49 for spring water, and 5.11 for deep-well water. The history of the water must, therefore, be known before its value can be ascertained. It is evident that there are special cases in which these quantities are enormously exceeded without danger.

The estimation of nitrogen as nitrates and nitrites is not difficult, and may be effected in one of three ways. Each is described at some length in Dr. Frankland's book, and each has its own advantages. He prefers that which involves the decomposition of the salts into nitric oxide, and the measurement of the gas evolved.

The methods for determining hardness that are suggested involve nothing new, and they have been too often described to require notice here.

The method of determining organic purity by the use of permanganate of potash, originally suggested by Professor Forchhammer in 1850, approved by Dr. Miller and other eminent chemists, and brought into use by the late Dr. Letheby, has been perfected by Dr. Tidy, and appears to produce results so satisfactory, as compared with the combustion process, when carried on under the most favorable conditions, that Dr. Frankland admits its usefulness and general accuracy in waters of moderate purity. It is elaborately described by Dr. Tidy in his memoir, and its advantages discussed. The prominent objections to the combustion process, which is still regarded by Dr. Frankland as the only secure method of determining the organic elements, will also be found fully stated in that memoir.

It must not be supposed that the analytical determination of the foreign substances present in water is sufficient to justify a conclusion as to the quality of the water without a due consideration of all circumstances, not only those indicated by the association of the elements, but those under which the water has or may have acquired them. No chemist, however able and intelligent, is justified in giving an opinion as to water submitted for analysis without knowing the history of the water, except, of course, where there are definite poisons present which enforce an absolute condemnation.* Neither the ammonia nor the nitrogen, neither the salt nor the hardness, may be regarded alone, without reference to this history. Thus it is that while mere analysis is easy, the estimation of waters for sanitary purposes must always require very great judgment as well as long experience.

Dr. Frankland gives in an appendix a number of typical

analyses of waters of various kinds and of various qualities. Adopting a classification suggested originally by Dr. Parkes, and modified by Dr. Tidy, he groups all waters into two sections—upland surface waters, and waters other than upland—and each section he divides into four classes—viz., waters of great purity, of medium purity, of doubtful purity, and of no purity at all; determining the value in each case by the permanganate process. This classification may be useful in some cases, but it is hardly of general application, inasmuch as "upland surface waters" is a very vague expression. The analyses given in this appendix are valuable, and are, we believe, chiefly quoted from the celebrated Sixth Report. We venture to suggest that they would be less liable to misconstruction if the estimate of what in this work Dr. Frankland still calls "previous sewerage contamination" were left out. This expression is, no doubt, explained (see pages 95-98.) as it has often been, and to those who understand the explanation it really means nothing that in any way affects the value of the water; but when we are told that rain water falling in London on November 8, 1873, contained 1,490 parts in 10,000 of this mysterious essence—that the deep-well water from the magnesian limestone contains, on an average, nearly ten times as much, and the upland surface water from the lower London tertiary none at all, we confess to a feeling of wonder that so misleading a title should continue to be used in reference to waters whose real value for dietetic purposes is not, and cannot be, in the smallest degree, influenced by so ugly an expression. The term has been withdrawn from the official returns describing the state of the London water, and it would be well if it could be expunged from the literature of analytical chemistry.

We have already alluded to some of the reasons of Dr. Tidy for rejecting Mr. Wanklyn's "albumenoid ammonia" process, and have pointed out that they are fully recognized by Dr. Frankland. This method is, however, by much the easiest of all for determining the organic constituents and for that reason is very widely adopted. It is described in detail in Mr. Wanklyn's volume already referred to, and one of the means of determination involves the precise comparison of shades of color. A possibility of personal error is thus introduced, which detracts very seriously from the value of a method which appears in other respects to be doubtful in its conclusions. We are not aware that Mr. Wanklyn has replied to the objections to his method, but we observe that he still adopts it in his determination of the organic contents of doubtful waters. The best, easiest, and safest method of estimating organic matter is, no doubt, the great problem to be solved in water analysis; but as it is not agreed whether the combustion process, the permanganate or oxygen process, or the albumenoid process, is the right one, it would seem reasonable that in all cases of dispute the analytical chemist on each side should be expected to give his results, not only in the way he thinks best, but also in the terms adopted by his *confrère*, and in such form that they admit of immediate comparison. If no other agreement can be arrived at, we may at least expect so much, and we think that in time it might be found possible to obtain, by common consent, a middle way that should satisfy all parties. At any rate and first of all, there might be a consensus in the matter of arithmetic.

THE FLY-TRAP—ITS FIRST DISCOVERY.—The Fly-trap (*Dionaea muscipula*) has lately been much spoken of; so it will be interesting to learn when this plant was first made known. John Ellis (1711-1776), a London merchant, received in 1769, from Philadelphia, the plant, and described it with drawings in "Directions for bringing over Seeds and Plants from the East Indies and other Distant Countries in a State of Vegetation, to which is added the Figure and Botanical Description of *Dionaea muscipula*," London, 1770. The same gentleman published in 1771, "Copies of two letters to Dr. Linnæus and Mr. W. Aiton," containing descriptions and drawings of two other North American plants, *Illitium floridanum* and *Gordonia lasianthus*.

FRED. BRENDL.

* It is well that this should be borne in mind by engineers and others who occasionally send waters for analysis with merely a number or other private reference. It is not fair to the chemist to require an opinion as to the goodness of water, without communicating its source and the circumstance under which it has been obtained. Generally the chemist should take the sample himself.

ON THE DYNAMO-ELECTRIC CURRENT AND ON CERTAIN MEANS TO IMPROVE ITS STEADINESS.

BY C. WILLIAM SIEMENS, D.C.L., F.R.S.

The author, after alluding to the early conception by Dr. Werner Siemens of the dynamo-electric or accumulative principle of generating currents, makes reference to the two papers on the subject presented, the one by Sir Charles Wheatstone and the other by himself, to the Royal Society in February, 1867. The machine then designed by him, and shown in operation on that occasion, is again brought forward with a view of indicating the progress that has since taken place in the construction of dynamo-electrical machines, particularly those by Gramme and Siemens von Alteneck. The paper next points out certain drawbacks to the use of these machines, both of them being subject to the disadvantage that an increase of external resistance causes a falling off of the current; and that, on the other hand, the short circuiting of the outer resistance, through contact between the carbon electrodes of an electric lamp, very much increases the electric excitement of the machine, and the power necessary to maintain its motion, giving rise to rapid heating and destructive sparks in the machine itself.

An observation in Sir Charles Wheatstone's paper is referred to, pointing to the fact that a powerful current is set up in the shunt circuit of a dynamo-electric machine, which circumstance has then been taken advantage of to some extent by Mr. Ladd and Mr. Brush, in constructing current generators.

The principal object of the paper is to establish the conditions under which dynamo-electric machines worked on the shunt principle can be made to give maximum results. A series of tables and diagrams are given, the results of experiments conducted by Mr. Lauckert, electrician, employed at the author's works, which lead up to the conclusion that, in constructing such machines on the shunt principle, the resistance on the rotating helix has to be considerably reduced by increasing the thickness of the wire employed, and that on the magnets has to be increased more than tenfold, not by the employment of thin wire, but by augmenting the length and weight of coil wire employed.

The results of this mode of distributing the resistances is summarized as follows:

1. That the electro-motive force, instead of diminishing with increased resistance, increases at first rapidly, and then more slowly towards an asymptote.
2. That the current in the outer circuit is actually greater for a unit and a half resistance than for one unit.
3. With an external resistance of one unit, which is about equivalent to an electric arc, when thirty or forty webers are passing through it, 2.44 horse power is expended, of which 1.29 horse power is usefully employed, proving an efficiency of 53 per cent., as compared with 45 per cent. in the case of the ordinary dynamo-machine.
4. That the maximum energy which can be demanded from the engine is 2.6 horse power, so that but a small margin of power is needed to suffice for the greatest possible requirement.
5. That the maximum energy which can be injuriously transformed into heat in the machine itself is 1.3 horse power, so that there is no fear here of destroying the insulation of the helix by excessive heating.
6. That the maximum current is approximately that which would be habitually used, and which the commutator and collecting brushes are quite capable of transmitting.

Hence the author concludes that the new machine will give a steadier light than the old one, with greater average economy of power; that it will be less liable to derangement, and may be driven without variation of speed by a smaller engine; also that the new machine is free from all objection when used for the purposes of electro-deposition.

This construction of machine enables the author to effect an important simplification of the regulator to work electric lamps, enabling him to dispense with all wheel and clock work in the arrangement. The two carbons being pushed onward by gravity or spring power, are checked laterally by

appointed metallic abutment situated at such a distance from the arc itself, that the heat is only just sufficient to cause the gradual wasting away of the carbon in contact with atmospheric air. The carbon holders are connected at the iron core of a solenoid coil, of a resistance equal to about fifty times that of the arc, the ends of which coil are connected to the two electrodes respectively. The weight of the core (which may be varied) determines the force of the current that has to pass through the regulating coil in order to keep the weight in suspension, and this in its turn is dependent upon the resistance of the arc. The result is that the length of the arc is regulated automatically, so as to maintain a uniform resistance signifying uniform development of light.
—*Engineering.*

THE HELIOGRAPH.

The English Government is again indebted to the Heliograph for the receipt of an important dispatch received from the seat of war in Afghanistan, announcing the result of an attack on British troops, in which their enemy suffered severely. The message is dated Camp Ghuzni, April 22d, and was received in London on the following day.

The value of the heliograph in war operations is becoming more apparent every day; in this instance the message probably could not have been delivered so speedily by electric telegraph. The Heliograph does not require the route to be kept open. The line of communication can not be cut, for the simple reason that the signalling takes place over the heads of the enemy, and the stations required are few and far between.—A ten inch mirror, and this is the size of the ordinary field heliograph, is capable of reflecting the sun's rays in the form of a bright spot or flare to a distance of fifty miles, the signal at this interval being recognizable without the aid of a glass. That is to say two trained sappers, each provided with a mirror can readily speak to one another, supposing the sun is shining, within an interval of fifty miles between them, provided their stations are sufficiently high, and no rising ground intervenes to stop the rays. The adjustment of the military heliograph is a very simple matter.—An army leaves its base where a heliograph station is located, and after travelling some miles desires to communicate with those they have left. A hill in the locality is chosen and a sapper ascends with his heliograph, which is simply a stand bearing a mirror, swung like the ordinary toilet glass, except that besides swinging horizontally, it is also pivoted, so as to also move vertically. Behind the mirror in the very centre, a little of the quicksilver is removed, so that the sapper can go behind the instrument and look through a tiny hole in it towards the station he desires to signal.

Having sighted the station by adjusting the mirror, he next proceeds to set up in front of the heliograph a rod, and upon this rod is a movable stud. This stud is manipulated like the foresight of a rifle, and the sapper, again standing before his instrument, directs the adjustment of the stud, until the hole in the mirror, the stud, and the distant station are in line. The heliograph is then ready to work, and in order to flash signals, so that they may be seen at a distance, the sapper has only to take care that his mirror reflects the sunshine, on the stud just in front of him.

A WONDERFUL METEOR.

MACON, GA., June 30, 1880.

At about twelve o'clock last night a meteor as large as a barrel, starting from the zenith, plunged down the north-eastern sky and exploded near here with a report that reverberated for thirty seconds and shook the earth even at this point. The meteor was about five seconds in falling, during which time the city was lit up as though by a powerful electric light. Much excitement prevailed in the negro quarter—the inhabitants rushing into their houses, closing the doors and filling the air with screams and prayers. The time between the disappearance of the phenomenon and the report was about three minutes. This would make the distance from Macon about forty miles.

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BY C. WILLIAM SIEMENS, D.C.L., F.R.S.

The author, after alluding to the early conception by Dr. Werner Siemens of the dynamo-electric or accumulative principle of generating currents, makes reference to the two papers on the subject presented, the one by Sir Charles Wheatstone and the other by himself, to the Royal Society in February, 1867. The machine then designed by him, and shown in operation on that occasion, is again brought forward with a view of indicating the progress that has since taken place in the construction of dynamo-electrical machines, particularly those by Gramme and Siemens von Alteneck. The paper next points out certain drawbacks to the use of these machines, both of them being subject to the disadvantage that an increase of external resistance causes a falling off of the current; and that, on the other hand, the short circuiting of the outer resistance, through contact between the carbon electrodes of an electric lamp, very much increases the electric excitement of the machine, and the power necessary to maintain its motion, giving rise to rapid heating and destructive sparks in the machine itself.

An observation in Sir Charles Wheatstone's paper is referred to, pointing to the fact that a powerful current is set up in the shunt circuit of a dynamo-electric machine, which circumstance has then been taken advantage of to some extent by Mr. Ladd and Mr. Brush, in constructing current generators.

The principal object of the paper is to establish the conditions under which dynamo-electric machines worked on the shunt principle can be made to give maximum results. A series of tables and diagrams are given, the results of experiments conducted by Mr. Lauckert, electrician, employed at the author's works, which lead up to the conclusion that, in constructing such machines on the shunt principle, the resistance on the rotating helix has to be considerably reduced by increasing the thickness of the wire employed, and that on the magnets has to be increased more than tenfold, not by the employment of thin wire, but by augmenting the length and weight of coil wire employed.

The results of this mode of distributing the resistances is summarized as follows:

1. That the electro-motive force, instead of diminishing with increased resistance, increases at first rapidly, and then more slowly towards an asymptote.
2. That the current in the outer circuit is actually greater for a unit and a half resistance than for one unit.
3. With an external resistance of one unit, which is about equivalent to an electric arc, when thirty or forty webers are passing through it, 2.44 horse power is expended, of which 1.29 horse power is usefully employed, proving an efficiency of 53 per cent., as compared with 45 per cent. in the case of the ordinary dynamo-machine.
4. That the maximum energy which can be demanded from the engine is 2.6 horse power, so that but a small margin of power is needed to suffice for the greatest possible requirement.
5. That the maximum energy which can be injuriously transformed into heat in the machine itself is 1.3 horse power, so that there is no fear here of destroying the insulation of the helix by excessive heating.
6. That the maximum current is approximately that which would be habitually used, and which the commutator and collecting brushes are quite capable of transmitting.

Hence the author concludes that the new machine will give a steadier light than the old one, with greater average economy of power; that it will be less liable to derangement, and may be driven without variation of speed by a smaller engine; also that the new machine is free from all objection when used for the purposes of electro-deposition.

This construction of machine enables the author to effect an important simplification of the regulator to work electric lamps, enabling him to dispense with all wheel and clock work in the arrangement. The two carbons being pushed onward by gravity or spring power, are checked laterally by

appointed metallic abutment situated at such a distance from the arc itself, that the heat is only just sufficient to cause the gradual wasting away of the carbon in contact with atmospheric air. The carbon holders are connected at the iron core of a solenoid coil, of a resistance equal to about fifty times that of the arc, the ends of which coil are connected to the two electrodes respectively. The weight of the core (which may be varied) determines the force of the current that has to pass through the regulating coil in order to keep the weight in suspension, and this in its turn is dependent upon the resistance of the arc. The result is that the length of the arc is regulated automatically, so as to maintain a uniform resistance signifying uniform development of light.
—*Engineering.*

THE HELIOGRAPH.

The English Government is again indebted to the Heliograph for the receipt of an important dispatch received from the seat of war in Afghanistan, announcing the result of an attack on British troops, in which their enemy suffered severely. The message is dated Camp Ghuzni, April 22d, and was received in London on the following day.

The value of the heliograph in war operations is becoming more apparent every day; in this instance the message probably could not have been delivered so speedily by electric telegraph. The Heliograph does not require the route to be kept open. The line of communication can not be cut, for the simple reason that the signalling takes place over the heads of the enemy, and the stations required are few and far between.—A ten inch mirror, and this is the size of the ordinary field heliograph, is capable of reflecting the sun's rays in the form of a bright spot or flare to a distance of fifty miles, the signal at this interval being recognizable without the aid of a glass. That is to say two trained sappers, each provided with a mirror can readily speak to one another, supposing the sun is shining, within an interval of fifty miles between them, provided their stations are sufficiently high, and no rising ground intervenes to stop the rays. The adjustment of the military heliograph is a very simple matter.—An army leaves its base where a heliograph station is located, and after travelling some miles desires to communicate with those they have left. A hill in the locality is chosen and a sapper ascends with his heliograph, which is simply a stand bearing a mirror, swung like the ordinary toilet glass, except that besides swinging horizontally, it is also pivoted, so as to also move vertically. Behind the mirror in the very centre, a little of the quicksilver is removed, so that the sapper can go behind the instrument and look through a tiny hole in it towards the station he desires to signal.

Having sighted the station by adjusting the mirror, he next proceeds to set up in front of the heliograph a rod, and upon this rod is a movable stud. This stud is manipulated like the foresight of a rifle, and the sapper, again standing before his instrument, directs the adjustment of the stud, until the hole in the mirror, the stud, and the distant station are in line. The heliograph is then ready to work, and in order to flash signals, so that they may be seen at a distance, the sapper has only to take care that his mirror reflects the sunshine, on the stud just in front of him.

A WONDERFUL METEOR.

MACON, GA., June 30, 1880.

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MICROSCOPY.

Dr. Henri Van Heurck, the well-known director of the Botanical Gardens at Antwerp, Belgium, proposes to issue a "Synopsis of the Belgian Diatoms," in a series of six numbers, each one to contain about a dozen plates. The division of plates will be such that the great groups of diatoms will be comprehended each in two parts, as follows: I and II, *Raphideæ*: *Amphoreæ*, *Cymbelleæ*, *Naviculeæ*, *Gomphonemææ*, etc.; III and IV, *Pseudo-Raphideæ*: *Epithemiceæ*, *Synedreeæ*, *Surirelleæ*, *Nitzschiceæ*, etc.; V and VI, *Crypto-Raphideæ*: *Melosireæ*, *Coscinodisci*, etc. This arrangement is that proposed by Prof. Hamilton L. Smith in the general synopsis of the Diatomaceæ as inserted in the "Traité du Microscope" (3e édition Bruxelles, 1878), de Mr. le Dr. Henri Van Heurck.

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H. L. SMITH.

HOBART COLLEGE, July 1, 1880.

NATURAL HISTORY.

Mr. Proctor remarks that among the problems with which science has not as yet succeeded in dealing satisfactorily is that of the flight of birds, and especially the flight of those birds which float for long periods of time without any apparent movement of their wings. During my voyage from San Francisco to Honolulu (which latter place, by the way, I have not reached at the moment of writing to the *Newcastle Weekly Chronicle*, 2.20 P. M., April 17th, ship time—lat. about 26° 35' and longitude about 145° west, so that Greenwich time is about midnight, April 17th) I have noted with much interest the flight of the birds—the sailors call them mollyhawks—which follow the ship apparently without ceasing, except for an occasional short rest on the water. It is certain that for many minutes together—in some cases I should say for fully ten minutes—these birds do not use their wings except to guide their movements and to sustain their bodies in the same sense that a parachute sustains a weight suspended to it—that is, they do not make active use of their wings, though of course a certain degree of muscular exertion must be involved in the mere sustentation of the body. I have seen nothing yet to confirm the statement I have often heard made, that these birds, albatrosses, and others, will float about, sustaining their bodies in this, as it were, passive manner during much longer periods of time, as an hour or so. I should be inclined to doubt whether a bird

could be, or has been, steadily watched even for half an hour. But if they do, the problem is not altered in character, but only in degree. Now it is manifest, in the first place, that the flight of a bird is not—as some who reject all attempts at explanation, would seem to imply—a miraculous phenomenon, but one purely dependent on ordinary mechanical laws. The muscular power shown by birds may be, and indeed is, very marvellous. The perfect adjustment of all their movements to obtain the greatest possible effect from every muscular effort, might probably be shown to be equally so, if we were able to analyze each movement as made, instead of being foiled as we are by the exceeding rapidity of a bird's evolutions. And, again, it is possible that the sustaining power of the air on bodies of particular form traveling swiftly through it may be much greater or very different in character from what has been hitherto supposed. But it is quite certain that the flight of birds depends on ordinary laws, however difficult it may be to explain it by their means. It may be a step towards the solution of the problem to consider what attempted explanations must at any rate be rejected. Amongst these is one which has been often advanced, and which seems to have a singular attraction for unscientific persons—the theory, namely, that the bones and quills of a bird are filled with some light gas, floating the bird in the same way that balloons are raised by the hydrogen gas within the silk. Those who hold this theory seem to imagine that hydrogen possesses some lifting power, as though the gas of itself sought to rise upwards from the earth. In reality, of course, hydrogen obeys the law of gravity and is drawn downwards, and not upwards. It rises much as the least heavily loaded scale of a balance rises—not because its own tendency is upwards, but because something else has a stronger tendency downwards. If a balloon instead of being filled with hydrogen were absolutely empty, and could yet retain its shape against the pressure of the surrounding air, it would rise more quickly than when filled with hydrogen, for the simple reason that it would be relieved of the weight of the hydrogen itself, which, though much smaller than that of an equal volume of air, still counts for something. Similarly, if the quills and hollow bones of the bird absolutely empty—no air nor the lightest gas being present in them—the lifting power resulting from this condition of things would be the greatest possible under the circumstances. A yet greater, in fact a very much greater lifting power would result if the whole body of the bird were hollow and vacuous. But how ineffective even this lifting power would be to raise the actual weight of the bird may be seen from the following simple considerations:—The specific gravity of a bird is certainly not less than a third that of water, as may be shown at once by observing how much of a bird's body is under water when the bird is floating. We may then safely assume that a bird's specific gravity is equal to 200 times the specific gravity of air. The difference then between the weight of the air displaced by a bird's body and the no-weight at all of an equal volume absolutely void, is only 1-200th part of the actual weight of the bird's body. This is the whole effective lifting force even in the perfectly imaginary case in which the entire volume of the bird is supposed to be available for this kind of support. The remaining 199-200ths, or practically the whole weight of the body, is left unsupported in this way, and some other explanation of the observed fact that it is supported remains to be sought for. I believe the true explanation is to be found in the enormous propulsive power of a bird's wings, combined with the perfect balance which the bird is able to maintain, with such changes only as may be rendered necessary by the changing direction of his motion. Of course I am aware that gravity acts with equal efficiency on a body traveling swiftly as on a body at rest. A cannon-ball allowed to fall from the mouth of a cannon reaches the earth no more quickly than one fired horizontally from the cannon's mouth. But I believe that a flat body travelling swiftly in a horizontal direction with its plane horizontal, sinks far more slowly earthwards than one of a similar shape which is not advancing or is only advancing slowly. The difference may be compared to that which would be noted between the fall of a flat stone on the surface of water when the stone is allowed simply to drop, and when it has been propelled horizon-

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tally along with great swiftness, so as to strike the surface skimmingly and travel along with the motion compared familiarly to the flight of ducks and drakes. I believe that when a bird has given to its body, by a few powerful propulsive efforts, a rapid forward motion, the resistance of the air resulting from the combination of perfect balance and swift forward motion suffices to explain all the phenomena of this floating movement. And I believe that if ever the art of flying—or rather of making flying machines—is attained by man, it will be by combining rapid motion with the power of perfect balancing.

GENERAL NOTES.

INSTRUMENTAL SUBSTITUTE FOR SINGING IN BIRDS.—The peculiar sound which the Lesser Spotted Woodpecker (*Picus minor*) makes upon trees by the action of its bill is extremely curious. It is quite certain that this habit has nothing whatever to do with the quest for food. The bird selects one particular spot upon the trunk or bough of a tree, which spot is naturally sonorous from the wood being more or less hollowed by decay. The bird returns to this precise spot continually during the day, and produces the sound by striking the wood on the spot with its bill, the stroke being repeated with a rapidity which is really incomprehensible; for it quite eludes the eye. It is effected by a vibratory motion of the head; but the vibrations are so quick that the action looks like a single stroke. After short pauses this stroke is again and again renewed, sometimes for several minutes together. During each interval the woodpecker looks around and below with evident delight, and with an apparent challenge of admiration. The beautiful crimson crest is more or less erected.

The whole performance evidently takes the place of the vernal song in other birds, and it is probably the only case among the feathered tribes in which vocal is replaced by instrumental music.

The nest is not usually in the same tree, but similar spots are selected on several trees in the neighborhood, and as the sound is very loud and heard a long way off, the hen bird, when sitting, is serenaded from different directions.

The above observations on the substitution of instrumental methods for singing among woodpeckers are highly interesting, and were made by the Duke of Argyle, and recorded in *Nature*.

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The pigment for the ground color is placed in a frame, in a solid block, and the design traced upon it. All the parts which do not form the ground color, are then cut out, and the spaces thus left are then filled by pouring in hot liquid pigments corresponding to the colors or shades required to be produced. When cold the recently added color is trimmed off with a knife, and another poured in, until the whole is built up. When complete the mosaic is placed in a press and the surface shaved by a knife, so as to make it true and level, and when moistened with suitable chemicals, it is ready for use. The impressions are clear, permanent and pass through the fabric.

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tally along with great swiftness, so as to strike the surface skimmingly and travel along with the motion compared familiarly to the flight of ducks and drakes. I believe that when a bird has given to its body, by a few powerful propulsive efforts, a rapid forward motion, the resistance of the air resulting from the combination of perfect balance and swift forward motion suffices to explain all the phenomena of this floating movement. And I believe that if ever the art of flying—or rather of making flying machines—is attained by man, it will be by combining rapid motion with the power of perfect balancing.

GENERAL NOTES.

INSTRUMENTAL SUBSTITUTE FOR SINGING IN BIRDS.—The peculiar sound which the Lesser Spotted Woodpecker (*Picus minor*) makes upon trees by the action of its bill is extremely curious. It is quite certain that this habit has nothing whatever to do with the quest for food. The bird selects one particular spot upon the trunk or bough of a tree, which spot is naturally sonorous from the wood being more or less hollowed by decay. The bird returns to this precise spot continually during the day, and produces the sound by striking the wood on the spot with its bill, the stroke being repeated with a rapidity which is really incomprehensible; for it quite eludes the eye. It is effected by a vibratory motion of the head; but the vibrations are so quick that the action looks like a single stroke. After short pauses this stroke is again and again renewed, sometimes for several minutes together. During each interval the woodpecker looks around and below with evident delight, and with an apparent challenge of admiration. The beautiful crimson crest is more or less erected.

The whole performance evidently takes the place of the vernal song in other birds, and it is probably the only case among the feathered tribes in which vocal is replaced by instrumental music.

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